

The path of the shadow defined by these numbers differs sensibly from that given in *NATURE*, vol. xv. p. 65. It passes to the north of Nimrud instead of the south.

With the permission of Mr. Hind, I also transmit the following list of solar eclipses, computed by him, of which the results are preserved in the manuscripts of the Royal Observatory:—

- 885, July 24 ...	- 533, Aug. 31 ...	A.D. 671, Dec. 6
- 884, July 13 ...	- 480, April 18 ...	647, Oct. 12
- 769, May 4-5 ...	- 479, Oct. 1-2 ...	840, May 5
- 762, June 14 ...	- 477, Feb. 16-17 ...	1133, Aug. 1
(above) ...	- 309, Aug. 14 ...	1140, March 20
- 688, Jan. 10 ...	- 189, March 13 ...	1173, June 11
- 609, Sept. 29 ...	- 187, July 16 ...	1241, Oct. 6
- 602, May 17 ...	- 103, July 18 ...	1433, June 17
- 584, May 28 ...	- 50, March 6 ...	1521, April 6-7
- 556, May 19 ...	A.D. 29, Nov. 23 ...	1567, April 8
- 548, June 18 ...	113, May 31 ...	1598, March 6
- 546, Oct. 22 ...	237, April 12 ...	1652, April 7
- 534, March 17 ...	418, July 18 ...	

G. B. AIRY

Royal Observatory, Greenwich, December 5

Negretti's Reversible Thermometer and the Arctic Expedition

CAPT. NARES presents his compliments to the editor of *NATURE*, and requests him to correct a mistake which Capt. Nares inadvertently made in his official report to the Admiralty concerning the late Arctic expedition, and which has been reprinted in *NATURE*.

In obtaining some deep-sea temperatures, which proved the existence of a sub-stratum of water warmer than that at the surface, the instruments used were the reversible thermometers of Negretti and Zambra, not Casella's. The Casella thermometer was used on other occasions, but not at the time referred to.

H. M. S. *Alert*, Portsmouth, November 30

The Arctic Expedition

TWO or three considerations have led me to believe that possibly the recent Arctic Expedition has not been so fortunate as might have been wished, and that the same amount of foresight, courage, and energy, expended on a similar expedition another year might be attended with much more satisfactory results.

The considerations referred to are these:—

Fifty years ago Sir Edward Parry traversed a distance of some hundreds of miles in sledges upon what he at first supposed to be the main pack; but on finding that as fast as he travelled northwards he was drifting to the south, he concluded what he had mistaken for the main pack, was after all only a loose floe of immense extent.

Now in 1872, on the return of the American expedition, we were all given to understand that an open Polar Sea existed where instead is now found a sea of ancient ice. All testimony concurred in pointing to this open sea. The climate was warmer than further south; birds were seen flying north; a creeping herbage flourished, and bright flowers were not absent. Musk oxen, rabbits, and lemmings also abounded. Now these *Polaris* explorers were neither mendacious nor stupid; and it seems to me that it is rather premature to set down their inference from all they observed as a mistake.

Now, sir, my theory, true or false, new or old, is this:—This Palæocrystic Sea is really a vast floating island of ice; say 500 miles in diameter. Just like the ice in a pail or on a pond, it melts in the hot weather at the edges, and then, disengaged from the land, it floats hither or thither, according to the direction of the prevalent winds or currents. If this theory be correct, it accounts for Parry's disappointing journey, for the inferences based on the *Polaris* observations, and for the otherwise unaccountable fact that the ice encountered by the recent explorers is undoubtedly ancient. The fact that the vast floe showed no signs of drifting away last summer only shows that the wind was unfavourable, or that this northernmost Greenland coast, when once the ice is stranded, does not easily relinquish its grasp. Possibly if a *Hecla* had attempted in 1876 what was impracticable in 1827, or if an *Alert* had tried in 1827 what has just proved a failure, both enterprises would have succeeded.

Next time two opposite routes must be undertaken simultaneously, of which one will fail and the other succeed.

WORDSWORTH DONNISTHORPE

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The Age of the Rocks of Charnwood Forest

IN reference to the letters which have appeared in *NATURE* (vol. xv. p. 97) allow me to say, in the first place, that I neither attached, nor intended to attach, any discredit to Mr. Woodward's very useful manual for the statements it contains in reference to the age of the Charnwood Forest Rocks. On the other hand, I am gratified to find that so competent an observer as the Rev. T. G. Bonney concurs with me in the view "that there is not a particle of evidence for their Laurentian age." This was the special point of my letter; and I fail to see that Prof. Green's hypothetical inferences from certain sections at Markfield (of which he fears that he has kept no record) are of much value in the question. Prof. Green, however, admits that the great mass of these rocks give no evidence of Laurentian age.

As regards what may be the respective limits of "Cambrian" and "Silurian" rocks that is another question. I am quite aware that Sedgwick claimed formations as "Cambrian" which are not so recognised by the Geological Survey, nor by the majority of authors, continental as well as British; for example, M. Barrande. To which of the series of formations belonging to the Cambrian system of Sedgwick the forest rocks are to be referred I am not prepared to say; but I think it must be allowed that the negative evidence founded on the absence of fossils ought to have some weight in favour of the view that they are referable to the horizon of the "Cambrians" of the Geological Survey rather than to that of the Llandeilo or Caradoc beds.

Mr. Bonney's comparison of the forest rocks with those of the volcanic series of the Lake District is very suggestive; nor is the correspondence of the strike of the beds in both districts without its weight, where every circumstance ought to be taken into consideration in question of such uncertainty. It would also be very desirable if some general understanding could be arrived at regarding the respective limits of the Cambrian and Silurian systems. There are scarcely two authors who adopt the same view on this subject. Theoretically it may be a matter of small consequence; but practically it gives rise to confusion amongst geologists and amongst students of geology. As this is the age of "conferences" why should not a conference of Palæozoic geologists meet and lay down a frontier line for the two kingdoms, which would last, perhaps, for a generation, and until the "instinct of nationality" crops up and brings on another conflict between the inhabitants of Cambria and Siluria, and their allies respectively?

EDWARD HULL

Geological Survey Office, Dublin, December 4

"Towering" of Grouse, Partridges, &c.

MOST of your readers doubtless know what is meant by the towering of game-birds; but, for the sake of those who do not, I will begin by describing the facts. When a partridge, for instance, is hit while on the wing by a few pellets of shot—perhaps only by one or two—the flight may continue for a variable distance; but, if the bird is a "towerer," a slight irregularity soon begins to show itself, after which the flight rapidly becomes more and more laboured, till eventually the bird ceases its onward motion altogether. The direction of the flight now changes from the horizontal to the perpendicular, and with a rapid fluttering sort of action the bird rises to a variable height, when all motion suddenly ceases, the animal falls like a stone, and the sportsman then knows that when he finds his partridge it will be lying dead on the exact spot where he "marked it down."

Before proceeding to state the cause of these curious movements, I should like to draw more prominent attention to the facts, first, that the time after receiving the wound during which horizontal flight continues is variable; second, that the limits of variation are tolerably definite, a bird never towering until it has flown some distance after being shot, and never flying any very great distance before towering; and third, that the height to which the bird rises is also variable, this height being sometimes only 1 or 2 feet, and at other times 40 or 50.

Now I suppose there is not a sportsman in the country who has not witnessed these phenomena scores and scores of times, and I dare say there is scarcely one of that numerous body who, if asked to assign the cause of these phenomena, would hesitate for a moment in his reply. From the time he first saw a bird tower, he has probably satisfied himself that the current hypothesis is the only one that can explain the curious facts; and if his interrogator should venture to doubt that cerebral injury is this cause, he would probably point to a drop of blood in the beak as a final answer to such scepticism. This drop of blood has doubtless always seemed to him such a complete verification of the current hypothesis, that he has probably never waited to ask himself the following questions:—1. Why is towering so common? The head of a partridge or grouse is a small object, and therefore not likely to be often hit. Moreover, common sense may show that if towering is due to cerebral injury, such injury must be of a very local and definite character as regards the brain: some particular part of that organ must be injured to the exclusion of all other parts, or else the effect would be instant death. This consideration would lead us to expect that towering, if it is due to cerebral injury, should be of exceedingly rare occurrence. 2. Why does a bird always fly some considerable distance before towering? If the action is due to cerebral injury, we should expect the former to ensue immediately upon the infliction of the latter. 3. Why is the distance which a bird flies before towering so variable? 4. Why is the height to which it does tower so variable? 5. Why is it that birds tower most frequently when shot from behind? 6. Why is it that we never see the hole in the skull through which the pellet has passed?

In view of these difficulties besetting the ordinary hypothesis, and in the hope of ascertaining the exact seat of cerebral injury if this hypothesis were the true one, I have last year and this year dissected a number of partridges which I had observed to tower, and in every case I found the cause of death to be the same, viz., pulmonary hæmorrhage. In all my specimens the lungs were gorged with extravasated blood. It thus becomes impossible to doubt that we have here the true cause of towering; and, as is always the case with true causes, examination will show that it is sufficient to explain all the effects. Towering is common, because the lungs expose a large area to receive the shot, and an area which is especially liable to be crossed by a single pellet from a bad marksman when, as is most usual, the bird is shot from behind. The bird always flies a considerable distance after being hit, because it takes time for the blood to pour into the spongy texture of the lungs from the open ends of the severed blood-vessels. The distance flown is variable, because it depends on the size and number of the severed blood-vessels—i.e., the rapidity of the bleeding—which of course is also variable. The height to which the bird towers is variable, because depending on the same cause. The drop of blood in the beak comes from the bleeding lungs through the wind-pipe—the latter organ in most of my dissections having been found full of clotted blood. Lastly, we do not find any indications, either externally or internally, of cerebral injury, for the simple reason that no such injury has taken place.

Any one who is not a physiologist may here ask, Why does pulmonary hæmorrhage give rise to such very peculiar movements as those that occur in towering? The answer must certainly be, that in these towering movements—which, be it remembered, only take place immediately before death—we have to do with the characteristically convulsive movements which in all animals mark the last stages of asphyxia. That in birds these movements should show themselves mainly in the wings, might, I think, be reasonably expected, seeing that the pectorals are the principal muscles in the body—and all sportsmen are aware how the particular birds in question exhibit violent fluttering motions of their wings when dying from any violent cause, just as rabbits, under similar circumstances, exhibit violent galloping motions of their principal muscle-masses in the hind legs. But why the convulsive movements of asphyxia should show themselves in these birds in the form of upward flight, is a question which I cannot answer. It seems, however, to be a question of some interest to the physiologist, and if worked out might possibly tend to elucidate that obscure subject, the mechanism of flight. Of course to investigate the phenomena of towering, asphyxia of birds would require to be produced in the laboratory; and here I must leave the matter in other hands, for although I have a licence to suffocate as many birds as I can in the pursuit of sport, I have no licence to suffocate a single bird in the pursuit of science.

And, in conclusion, may I suggest that those sportsmen who annually conduct their experiments on asphyxia by the thousand, should endeavour to glean from them one result of some little value to science? It would be of interest to know what birds tower and what birds do not. So far as my own observation extends, the peculiarity in question seems to be confined to members of the grouse genus, nearly all the endemic species of which I have observed to tower. But, excepting those species, I have never known any other bird to do so. By publishing this notice in your columns, therefore, I hope to obtain information from any of your readers who may have observed the well-known phenomena in birds of other genera. GEORGE J. ROMANES

Squirrels

ON the lawn before the window near which I am writing is erected a tripod of three lofty poles, at the summit of which is suspended a basket containing nuts and walnuts. The squirrels, of which there are many in the shrubberies and adjoining plantations, ascend these poles, extract a nut from the basket, and quickly make their way down and across the lawn, in various parts of which they bury their nuts, scratching a hole in the green turf, putting in a nut, filling up the hole, and, lastly, with much energy, patting the loose materials with their feet till the filling-up is made firm and solid. This morning for a considerable time only one squirrel was at work, giving me a better opportunity of observing the mode of operation. His journeys were made in all directions, and varied from 5 feet to nearly 100 yards, never, so far as I could observe, going twice to the same place or even nearly so. The squirrels, I am told, forget the spots where they hide the nuts, and in the following spring the lawn, which is very spacious, is dotted with the young plants of nuts and walnuts. As the colours of flowers attracting bees and moths promote fertilisation, so the racy flavour of a nut, irresistible to a squirrel, contributes to the distribution of its kind.

Turvey Abbey, November

HENRY H. HIGGINS.

Mr. Harris's Challenge to Mathematicians

IN an advertisement in NATURE (vol. xv., p. xxxviii.) Mr. Harris (Kuklos) challenges mathematicians "to examine and disprove if they can" his published demonstration of the value of π . Presumably he reads this publication; if so, we would direct his attention to an article on "Cyclometers and some other Paradoxes" in vol. xii., p. 560, vol. xiii., p. 28. The part which is concerned with his approximation will be found on p. 29. Reasoning, however, which we venture to think will satisfy mathematicians, may not, we fear, convince Mr. Harris.

THE WRITER OF THE ARTICLE

December 4

A ZOOLOGICAL STATION ON THE NORTH SEA

REFERENCE was made in NATURE (vol. xiv. p. 535) to the resolutions passed at the recent meeting of the Association of German Naturalists and Physicians at Hamburg, as to the establishment of zoologico-botanical stations on the German coast. The distinguished names of those appointed to draw up a memorandum, which is to be presented to the Imperial Chancellor, the Bundesrath, and the Governments of the several States of the Empire, will no doubt be of the greatest service in securing success to a scheme so universally approved of by all students of biology.

The following is a brief sketch of similar endeavours made in Holland a year ago and of the results arrived at during the summer of 1876:—The Netherlands Zoological Association, at a meeting held in November, 1875, recognised the necessity of founding an establishment on the Dutch coast, where anatomical and microscopical investigations of the fauna and flora of the North Sea might be carried on at leisure, and which could at the same time be made serviceable for physical, chemical, and meteorological